UNIVERSITY OF SOUTHERN CALIFORNIA
UNIVERSITY PARK
LOS ANGELES 7

September 27, 1948

Dr. J. Lederberg
Department of Genetics
University of Wisconsin
Madison 6, Wisconsin

Dear Dr. Lederberg:

The following is a summary of our experiences so far in our attempts to get recombination in various Salmonella organisms. We approached the problem somewhat differently than you, and were interested only in getting recombinations of the somatic antigens of two Salmonella "species." We worked with Salmonella cholerae-suis (VI VII) and Salmonella poona (XIII and XXII). Our original plan was to grow these organisms together and attempt to locate varieties that might have different combinations of these four antigens than the parent strain. We attempted three screening mechanisms in order to eliminate the parent cultures from our mixtures. The first approach was to grow our mixed cultures on agar containing various combinations of mono-specific antisera. We hoped that the growth of an organism containing a specific antigen would be suppressed in a medium containing the homologous antisera. With the proper combination of antisera in the medium only organisms with certain antigenic structures might be expected to grow. This approach turned out to be unworkable in our experience.

Our second approach was to prepare biochemical mutants of the two species of Salmonella and attempt to locate recombinants of a biochemical character hoping that in some cases we would also get recombinations in antigenic structure. Mr. Oppenheimer was working on this phase of the problem and his results also were completely negative. He was able to develop a cholerae-suis strain that required arginine and one other undetermined growth factor which he attempted to cross with his poona strain having two different metabolic needs. In no case was he able to obtain any evidence of a recombination. I do not feel that Oppenheimer's work was conclusive since he left us at the time when his work was just starting to click.

Our third approach was to attempt to screen out the parent organisms through the use of specific phages. We obtained a phage against cholerae-suis and a different one active against poona. We then grew these two organisms together for various periods of time and at intervals treated samples of the mixed growth with both phages. After allowing time either for absorption or for lysis the phage-treated suspensions were plated out. The antigenic structure of the organisms surviving this treatment was tested. We are practically certain that we have obtained a new type of Salmonella containing the combination of VI, VII and XIII, and also have evidence that some of the other possible combinations exist. Until we have some outside "expert" confirm the typing of our cultures we are a little hesitant in publishing these findings.

(Dr. Lederberg, continued)

There is nothing in the evidence we have so far to indicate that this "recombination" or "creation of a new species" can be interpreted positively as a genetic phenomenon. In fact our failure with the second approach and the technique of the third makes it equally probably, if not moreso, that we are dealing with something akin to type transformation in the pneumococci. We are attempting to expand our work as rapidly as possible in order to obtain some satisfactory explanation of these results. It may be that as the problem develops we could very profitably employ your strain of typhimurium in our further work, but we will know more about this after our next experiments. We will keep you informed of developments.

If for any reason you would desire transplants of the cultures or the phages we are using I would be very happy to furnish them to you.

In an entirely different connection I wonder if in any of your work you have ever run across a coli strain having a requirement for glutamic acid. I would appreciate receiving such an organism if one is in existence, along with the parent non-requiring strain.

Thank you again for your letter of July 31.

Yours sincerely,

S. C. Rittenberg

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